F.R. del.

Lowry. sculp.

DESCRIPTIONS

OF

AN ELECTRICAL TELEGRAPH,

WINDSOR CASTLE, MARCH 31.
The Queen was this day pleased to confer the honour of Knighthood on Francis Ronalds, Esq.

SOME OTHER ELECTRICAL APPARATUS.

SIR FRANCIS RONALDS.—The name of this gentleman, who had the honour of receiving knighthood from Her Majesty on Thursday last, the 31st of March, in consideration of his having been the original inventor of the electric telegraph, was spelt in our issue of Thursday "Ronolds," in mistake for "Ronalds." april 1870

FRANCIS RONALDS.

"Volenti nihil difficile."

LONDON:
PRINTED FOR R. HUNTER,
72, ST. PAUL'S CHURCHYARD.
1823.

On the 8th inst., at St. Mary's Villas, Battle, Sussex, Sir FRANCIS RONALDS, F.R.S., in the 86th year of his age.

LONDON:

PRINTED BY CHARLES WOOD,

Poppin's Court, Fleet Street.

Society of Telegraph Engineers.—The fourth session of this society was inaugurated on Wednesday night at the rooms of the Institution of Civil Engineers, Great George-street, Westminster. Mr. Latimer Clarke, the President of the year, occupied the chair, and delivered an interesting address on the early history and subsequent development of the electric telegraph. At the outset he alluded to several striking features connected with the invention of the telegraph, many of which were embraced in a recent article in *The Times* entitled "Fifty Years' Progress." He spoke of Sir Francis Ronalds as having lived some 30 years before his age, otherwise he might have had to be written the inventor of the electric telegraph. After carefully tracing the progress of telegraphy during the past 30 or 40 years, Mr. Clarke went on to speak of the great development the system has undergone since it was transferred to the Post Office in 1870. It is estimated that in the days of the Telegraph Companies the total number of messages in a year did not exceed six millions. But Mr. Clarke showed how in 1871 the number had been increased to 11,000,000, in 1872 to nearly 15,000,000, in 1873 to upwards of 17,000,000, and last year to 19,100,000. The number of miles of wire possessed by the Post Office at the present time was stated to be upwards of 106,000, in addition to nearly 1,500 miles of submarine cables. The number of postal telegraph offices opened at the present time is 5,572, and the number of instruments worked by the Post Office exceeds 9,200. Mr. Clarke pointed out that the Government, instead of retarding the progress of inventions, as was at one time feared, had done everything in its power to foster them, and was always prepared to receive practical suggestions, from whatever quarter they might be offered. Adverting to the history of the society, if a society can be said to have a history which has only been three years in existence, the President announced the gratifying fact that the members at the present time exceed 650, and that the annual revenue is nearly £1,000. This result is all the more wonderful when taken in connexion with the fact that the parent society, the Civil Engineers, was 30 years in existence before it possessed so large a membership. The society possesses a journal in which its proceedings are printed, and Mr. Clarke did not despair of the time when it would possess a laboratory and a system of experimental lines for the benefit of students, many of whom are members of the society. Their example, too, had been followed in America, where an Electrical Society had been formed in Chicago with every prospect of success. But perhaps the most interesting feature of the evening's proceedings was the announcement that the magnificent library of electrical works collected by the late Sir Francis Ronalds had been transferred in trust to the society, with reversion to the Royal Society in the event of the dissolution of the Society of Telegraph Engineers. This library, which is said to contain nearly 10,000 volumes, was bequeathed to Mr. Samuel Carter, brother-in-law of Sir Francis Ronalds, who was present at the meeting and elected an honorary member of the society. Mr. Clarke concluded by pointing out that the progress of telegraphy, whereby messages could be sent at greater speed than formerly, must ultimately lead to the cheapening of the rates, and suggested a special and higher rate for "express" messages, just as we have express trains at higher fares. This is a subject which has often engaged public attention, but it may fairly be doubted whether, with the national system of telegraphy which we now possess, any such system of special telegrams could be worked with advantage to the numerous interests concerned. A vote of thanks to Mr. Clarke for his admirable address was proposed by Mr. W. Hooper, the well-known cable manufacturer, and seconded by Professor Hughes, the well-known electrician, and inventor of the admirable instrument which bears his name.

obituary column yesterday contained the name of a gentleman long known as one of the foremost labourers in the field of applied science, Sir Francis Ronalds, F.R.S., formerly Director of the Observatory at Kew, at the age of 85. The son of the late Mr. Francis Ronalds, of Highbury, Middlesex, he was born in the year 1788, and received his early education at the Academy at Cheshunt. Although, with his usual modesty, he constantly disclaimed the honour often ascribed to him of having been the "inventor" of the electric telegraph, yet it remains on record that nearly 60 years ago he devised a perfectly efficient instrument of that kind, which he fully described in a pamphlet published in 1823. His first success in this direction is described as follows in the *Philosophical Magazine* :—"In the summer of 1816 he undertook to prove the practicability of telegraphic communication, at great distances, by transmitting a certain number of electric shocks, for an arranged signal, through insulated wires of considerable length. He laid his wire in glass tubes surrounded by wooden troughs lined with pitch, which were placed in a covered ditch, 525ft. long and 4ft. deep, dug in his garden at Hammersmith. He also suspended eight miles of wire, by silk cords, from two wooden frames erected on his lawn, so that the wire passed to and fro many hundred times, well insulated at each point of attachment, and forming one continuous line, kept separate from contact with other parts. Both these kinds of apparatus served equally to show the instantaneous transmission of the electric shock. In order to provide the means of conveying intelligence along the underground line, he placed at each end of it a clock, with a dial bearing 20 letters inscribed. In front of the dial was a disk, revolving with the seconds hand, forming a screen with a small opening cut in it, so that as the disk revolved only one letter could be seen at a time, and this only for a second. The two clocks were made to go isochronously, the one always presenting the same letter as the other at any given second of time; and the moment chosen at one end was indicated at the other by the sudden collapse of a pair of pith ball electrometers, suspended at each station close to the clock-dial and connected with the telegraph wire." By this contrivance, says a writer in the *Illustrated London News*, "letter after letter could be denoted, and words spelt out as certainly as by the telegraphic apparatus of Messrs. Wheatstone and Cooke, invented at a later period and patented in 1837." The same writer adds a fact which, read by the light of science at the present day, will cause a smile—namely, that when Mr. Ronalds proposed that a telegraph of his construction should be laid down between London and the residence of the Prince Regent at Brighton, and with that view submitted his project to Lord Melville, then at the head of the Admiralty, he received a curt official reply from "their Lordships" of that Department, intimating that "telegraphs of any kind are now wholly unnecessary, and that none other than that now in use will be adopted." The word "now," of course, referred to the recent end of the long European war and the fall of Napoleon. Sir Francis Ronalds was the original Honorary Director of the Royal Observatory at Kew (a post which he held from 1843 to 1852), and the inventor of various self-registering instruments which are employed there, as well as at the Royal Observatory at Greenwich, at the Radcliffe Observatory at Oxford, and in several foreign observatories, all of which were fully described by him at various times in memoirs and papers read before or published by the Royal Society, the British Association, &c., and also in a French pamphlet which accompanied their display at Paris in the Exhibition of 1855. In 1870 he received a somewhat tardy reward for his public services in the cause of science by being knighted, "in acknowledgment of his early and remarkable labours in telegraphic investigation." A scientific account of these will be found in his pamphlet published 50 years ago, to which we have already referred. He had already been for some years—we believe since 1852—in receipt of a pension on the Civil List.

Times Aug 1873

RONALDS ON AN ELECTRIC TELEGRAPH, &c.

ADVERTISEMENT.

It was the original intention of the author, on taking leave of a science, which once afforded a favourite source of amusement, to prepare for the press many other portfolio scraps, as little deserving of publicity as these, perhaps: but the short period which *could* be devoted to the task having expired, he is *already* compelled to bid a cordial adieu to Electricity; and to rely upon the indulgence of Electricians to excuse the bad style, and all those inaccuracies and omissions which *can* be attributed to an imperious haste in the composition of his little production.

Wigmore Street,
June 11, 1823.

CONTENTS.

INVENTION OF THE TELEGRAPH.—We are requested by Sir Francis Ronalds to state, in reference to a paragraph which appeared in our impression of the 4th inst. (of which he was not previously cognizant) that although he invented and employed a perfectly efficient electric telegraph in 1816, and fully described it in 1823, he disclaims the appellation of "original inventor of the electrical telegraph," many schemes of the kind having preceded his. The motive for Her Majesty's gracious act on the 31st ult. has been very appropriately designated by the Prime Minister an "acknowledgment of his early and remarkable labours in telegraphic investigation."

CHRIST'S HOSPITAL.—In the report of the meeting of governors of this hospital the mover of the amendment was described as Mr. Leatham. The mover was Mr. Latham.

DESCRIPTION

OF

AN ELECTRIC TELEGRAPH.

Dr. Watson and his friends, Lord C. Cavendish, Mr. Martin Foulks, Dr. Bevis, Mr. Graham, Dr. Birch, Mr. P. Duval, Mr. Trembly, Mr. Ellicot, Mr. Robins, Mr. Short, and some other gentlemen, so long ago as the year 1748, proved, that electrical shocks might be conducted through long circuits with *immeasurable* velocity. These indefatigable and ingenious experimentalists founded the present commonly received axiom amongst electricians, that *no* perceptible space of time is required to transmit electric signs through conductors of *any* extent; and Mr. Cavallo, in his "Complete Treatise on Electricity," has suggested a method of conveying intelligence, by passing *given numbers of sparks* through an *insulated* wire in given spaces of time. Some German and American savans first projected Galvanic or Voltaic telegraphs, by the decomposition of water, &c. But the other form of the fluid appeared to me to afford the most accurate and practicable means of conveying intelligence; and, in the summer of 1816, I *amused* myself by wasting, I fear, a great deal of time, and no small expenditure, in trying to prove, by experiments on a much more extensive scale than had hitherto been adopted, the validity of a project of this kind. I believe I succeeded to the entire satisfaction of several very eminent scientific friends; and I am sure that *they* will at least acquit me of wishing to claim the smallest share of originality which does not belong to them. The *result seemed* to be, that that most extraordinary fluid or agency, electricity, may actually be employed for a more practically useful purpose than the gratification of the philosopher's inquisitive research, the schoolboy's idle amusement, or the physician's *tool;* that it may be compelled to travel as many hundred miles beneath our feet as the subterranean ghost which nightly haunts our metropolis, our provincial towns, and even our high roads; and that in such an enlightened country and obscure climate as this its travels would be productive of, at the least, as much public and private benefit. Why has no *serious trial* yet been made of the qualifications of so *diligent* a courier? And if he should *be* proved competent to the task, why should not our kings hold councils at Brighton with their ministers in

London? Why should not our government govern at Portsmouth almost as promptly as in Downing Street? Why should our defaulters escape by default of our foggy climate? And since our piteous inamorati are not all Alphei, why should they add to the torments of absence those dilatory tormentors, pens, ink, paper, and posts? Let us have *electrical conversazione offices*, communicating with each other all over the kingdom, *if we can.*

However evident it may have appeared to Dr. Watson, and the savans of his time (who drew their conclusions from experiments, the most satisfactory of which was that performed upon a circuit of two miles, placed in a field near Shooter's Hill), that a conductor of *any* extent, or *capacity*, could be made to *discharge* itself *instantaneously*, when brought into contact with the earth, or with an oppositely electrified surface of a certain capacity, it will not appear, to those who have well considered the beautiful experiments of the later Italian electricians, immaterial to show, that through a distance of eight miles of insulated wire no perceptible space of time is required to transmit an electrical sign from one extremity to the other; and I hope, that the following method of proving the fact will be deemed rather more satisfactory than the Doctor's; since his men (had they been furnished with nerves of the most perfect tone) having *first* to receive a strong shock, and also to see a spark, must have employed *some* time to perform their task. But here I would beg leave, rather than occupy those readers' attention with a subject which may not immediately interest them, to refer those whom it may, to the works of that "Princeps in re electrica," Signior Volta, whose valuable remarks and observations on insulation and compensation have not, perhaps, yet obtained all the attention which they merit.

Upon a lawn or grass plot at Hammersmith, I erected two strong frames of wood, A and B, Plate 1, at the distance of twenty yards from each other, and each containing nineteen horizontal bars. To each bar was attached thirty-seven hooks, and to the hooks were applied as many silken cords, which supported a small iron wire (by these means well insulated) which (making its inflexions at the points of support) composed, in one continuous length, a distance of rather more than eight miles.

When a Canton's pith ball electrometer was connected with each extremity of this wire, D and E, and it was *charged* by a Leyden jar, F, both electrometers appeared to diverge suddenly at the same moment; and when the wire was *discharged*, by being touched with the hand, both electrometers appeared to collapse as suddenly.

When any person took a shock through the whole length of wire, and the shock was compelled to pass also through the two insulated inflammable air pistols, C, one connected with each extremity of the wire, D and E, the shock and the explosion seemed to occur quite simultaneously.

But when the spark was compelled to pass through the gas pistols, C, and any one closed his eyes, it was *impossible* to distinguish more than one explosion, although both pistols were discharged.

When people did not look at the pistols, and when I sometimes charged only one highly, and sometimes both lowly, they could never guess, except by mere chance, whether one or both were fired.

Thus, then, three of the senses, *viz.* sight, feeling, and hearing, seemed to receive absolute conviction of the *instantaneous* transmission of electric signs through my pistols, my eight miles of wire, and my own proper person. Yet I do not contend, nor even admit, that an *instantaneous discharge*, through a wire of *unlimited* extent, would occur in *all* cases; for, in short, I think that Volta has clearly shown, that *compensation* may *sometimes* be made to amount to the destruction of a discharge.

However, this set of experiments appeared to afford no grounds for abandoning the project of an electric telegraph; and the following is the method which I chose, out of several others which I successfully tried, as being the most *convenient.*

A trench was dug in the garden, five hundred and twenty-five feet in length, and four feet deep. In this was laid a trough of wood, two inches square, well lined in the inside and out with pitch, and within this trough thick glass tubes were placed, through which the wire ran. Fig. 1, Plate 2, represents a section of the trough, tubes, and wire; and it will be seen, that the different lengths of tube, A, B, C, did not touch each other, but that at each joint, or rather interval, other short tubes, D, E, were placed, of just sufficient calibre to admit the ends of the long ones, together with a little *soft* wax, which effectually excluded any moisture from insinuating itself between them, and yet allowed a little play or motion for the contraction and expansion which variations of temperature produced; for in the first attempts, when *hard* cement was used, the tubes were apt to break by such changes of temperature. The trough was then covered with pieces of wood, screwed upon it whilst the pitch was hot; they also were well covered with pitch, and the earth then thrown into the trench again.

A light, circular brass plate, Fig. 2, divided into twenty equal parts, was fixed upon the seconds arbor of a clock which beat dead seconds.

Fig. 2.

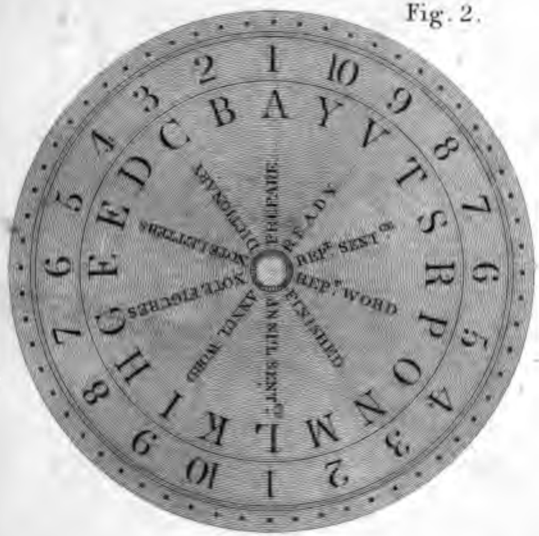

Fig. 3.

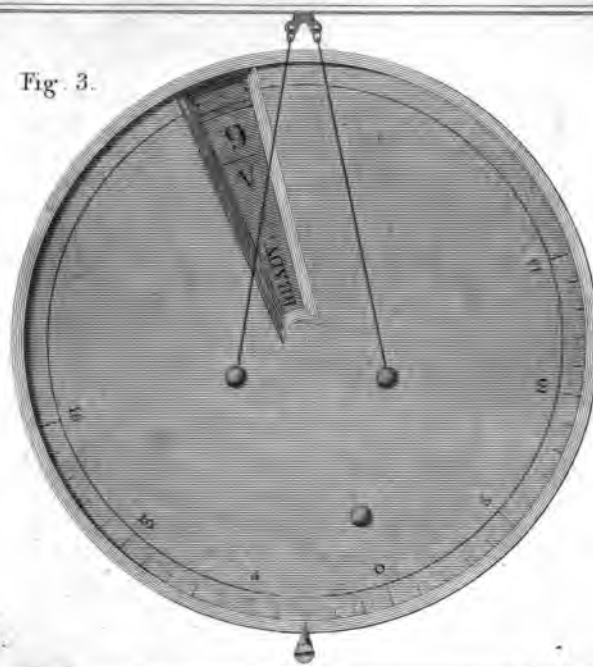

Fig. 1.

F. R. delt.

Lowry, sculp.

to go as nearly as possible synchronically with the first.

Hence it is evident, that when the wire was *charged* by the machine at *either* end, the electrometers at *both* ends diverged: when it was *discharged* suddenly at either station, they both collapsed at the same instant; and when it was discharged at the moment when a given letter, figure, and sign on the lower plate, Fig. 2, Pl. 2, of one clock appeared in view through the aperture, the same figure, letter, and sign appeared also in view at the other clock; and that by such *discharges* of the wire at one station, and by noting down the letters, figures, or signs *in view* at the other, any required words could be spelt and figures transmitted.

But, by the use of a telegraphic dictionary, a word, or even a whole sentence, could be conveyed by only three discharges, which could be effected, in the shortest time, in nine seconds, and in the longest in ninety seconds, making a mean of fifty-four seconds. This dictionary consisted of ten leaves, one of which is represented by Plate 4, numbered on the right hand, and cut in the manner of a common-place book or ledger: each leaf was also divided into ten columns, and each column numbered on the top of the page. The columns were intersected by ten horizontal lines, each numbered on the left side; and the spaces produced by the intersections were occupied by words or sentences.

It was necessary to distinguish the preparatory signs from those intended to spell, or refer to the dictionary, by giving the wire a rather higher charge than usual, and thus cause the pith balls to diverge more; and it was always understood, that the first sign, *viz.* "PREPARE," was made when that word, the letter A, and figure 1, were *in view* at the *communicator's* clock; so that, should the communicant's clock not exhibit the same sign (in consequence of its having gained or lost more than the communicator's) he noted how many seconds it had lost or gained, and moved his upper plate on its centre through just so many seconds, either to the right or left, as occasion required; and the communicator continually repeated his sign "PREPARE" until the communicant had adjusted his clock, and had discharged the wire, at the moment when the word "READY" appeared in view.

A second preparatory sign was now made by the communicator, provided that the word or sentence was not contained in the dictionary, or that the figures were to be noted, not as referring to the dictionary, but in composition; and this was done by discharging the wire at the moment when the term "NOTE LETTERS," or "NOTE

FIGURES," came in view. Other preparatory signs were sometimes required, which were transmitted in the same manner, and may be easily understood by reference to the drawing.

In making use of the dictionary, it was also always understood, that the first sign referred to the number of the leaf written on the right hand of the same; the second, to the number of a column at the head of the page; and the third, to one of the horizontal lines. Thus, supposing that the figures 9, 2, 4, were transmitted, they would immediately indicate the sentence, "STEAM PACKET ARRIVED FROM."

The gas pistol, F, Plate 3, which passed through the side of the clock-case, G, was furnished with an apparatus, H, by means of which a spark might pass through it when the communicator made the sign "PREPARE," in order that the explosion might excite the attention of the communicant, and thus obviate the necessity of such continually *close watching* as is necessary in the telegraph now in use; and the handle, I, enabled him to break the connection of it with the wire when necessary.

At half the distance between the two ends of the wire was placed the apparatus K, by which its continuity could be broken at pleasure, for the purpose of ascertaining (in case any accident had happened to injure the insulation of the buried wire) *which* half had sustained the injury, or if *both* had. It is seen, that the two portions of wire and tube rose out of the earth, and terminated in two clasps or forks, L and M; and the wire N, carrying a pair of pith balls, resting in these forks, connected them. Now by detaching this connecting wire from the fork L, whilst it still remained in contact with the fork M, or *vice versa,* it could be seen which portion of the wire did not allow the balls of the electrometer to diverge, and consequently which had lost its power of insulation, or if both had. But I never had occasion to make use of this contrivance after using soft wax instead of hard cement to unite the different lengths of glass tube.

One of the stations was situated in a room over a stable, and the other at the end of the garden, a distance (as I have said) of five hundred and twenty-five feet, in a tool-house; and the wire was buried under a gravel walk.

Having now given a general idea of the disposition of the apparatus, and its application to the purpose of a telegraph, on a small scale, *exactly as it was constructed and employed,* it will not be necessary to trouble the reader with any minor details. But it still remains to *attempt* the removal of a few objections to its construction and *useful* application on a large scale, and to state some few improvements, which time and reflection have suggested.

1. That objection, which has seemed to most of those with whom I have conversed on the subject the least obvious, appears to me the most important, therefore I begin with *it;* viz. the probability, that the electrical compensation, which would take place in a wire enclosed in glass tubes of many miles in length (the wire acting, as it were, like the interior coating to a battery) *might* amount to the *retention* of a charge, or at least might destroy the *suddenness* of a discharge, or, in other words, it might arrive at such a degree as to retain the charge with more or less force, even although the wire were brought into contact with the earth.

As an attempt to obviate this difficulty, let a trial be made (of the same nature as the above-described experiment with the eight miles of wire, see page 4), upon another similarly circumstanced to that which was buried in the garden, in order to prove how far it would conduct a sign of positive electricity *instantaneously.* Let the wire be then still prolonged to a given extent, and its discharge made into a battery of a given capacity, charged negatively to a given tension, instead of the discharge being made, as before, into the earth. An Epinus, a Cavendish, a Lord Stanhope, a Haüy, or a Coulomb, would by such means procure data, whereby to determine mathematically what the size of a battery, charged to a given intensity negatively, should be, to produce or receive an instantaneous discharge from any required length of the buried wire, charged also to a given intensity positively. But an experimental solution of the problem would certainly be preferred, because there are circumstances, such as the deposition of moisture from the atmosphere on the jars, the state of the earth in respect to humidity, &c. &c., which would materially affect the insulating power of the glass, and the conducting powers of the earth and wood in the trench, and which, being subject to perpetual and great variations, could not be justly appreciated in calculation. But *every* electrician will see, that this expedient *should* enormously increase the power of procuring a sudden discharge, and that it might, comparatively speaking, be carried to an unlimited extent*.

* Dr. Pearson first decomposed water by small electric sparks: has it ever been attempted to do so by that gradual discharge of a very large battery, which may be effected through a silken thread, or any similar substance containing a slight degree of moisture? The stream of electricity passing out of such a thread, &c., very much resembles in appearance that from the Voltaic apparatus; and I have sometimes made it last three or four seconds. Perhaps such a stream might be more efficient in certain decompositions than the Voltaic

Let the glass tubes, inclosing the wire, be very thick; for an electric charge is retained with a force increasing or decreasing as the squares of the distances of the two opposed surfaces. Let them be covered with wax, or varnish, or any non-conducting substance, which attracts moisture but slightly. Let them be raised from the bottom of the trough by supports of wood, or (better) glass (see Fig. 1, Plate 2); and let moisture be excluded from the trough as effectually as possible; for then both the moisture and the bottom of the trough will furnish a much less effective exterior coating to the tubes, and consequently there will be less compensation. Perhaps cast iron

streams. Every one must have observed the short, whizzing sound, which occurs when the inside coating of a battery is approached by a conductor, not in metallic or good conducting communication with the outside; a sound very different from the loud snap, or explosion, which takes place when the discharge is made through a complete metallic circuit. This more gradual whizzing discharge, accompanied by intolerable pain, often happens when a battery, standing upon a very dry old table, is approached by the knuckle. I have seen straw conductors in France, for the preservation of buildings, which have been adopted on this principle instead of points; and I once discharged a battery through a card, with the intention of producing Lullin's double burr, when this whizzing noise was heard to last about one second, but no burrs appeared; and, on examining the card with a microscope, a very minute puncture appeared to have been effected through it, the edges of which were burnt.

troughs might be rendered as tight as gas pipes.

It seems probable enough, that an instantaneous discharge might be obtained from a wire of any *required* length by the above-mentioned adoption of means, which seem capable (as has been said) of almost unlimited application. But if upon trial (the only fair test of such like contrivances) it should be found otherwise, or the expenditure disproportionate to the value of the object proposed, *viz.* a saving of time, then intermediate stations might be employed, as is the case with telegraphs now in use.

Or supposing, that even a whole minute should be required to transmit a single sign from Carlton House to the Pavilion at Brighton, an easy mode might be found to allow for that minute in the adjustment of the clocks; and may we not fairly conclude, that since our eight miles of wire insulated in the air required *no* perceptible space of time to transmit a sign from one end to the other, the length of fifty miles of buried (but still little compensated) wire, would not require *more* than one minute to perform its task? Were the time occupied five minutes, I should count this objection rather serious, but not insurmountable.

2. The liability of so fragile a substance as glass to be broken *by accident*, has appeared to some a much more important objection.

It may be replied, that I adopted glass as being a substance more durable, and *less* liable to accident than Mr. Cavallo's pitch and cloth; yet his method answered the purpose very well. After using the *short* tubes of glass and wax to unite the *other* tubes, I never found them liable to break or give way, during half a year's trial; and they remain at this day in as perfect a state as they were on the first that they were used, *I believe.* Should, however, the other method be proved best, let it be employed. No person of competent experience in these matters will doubt, that either of them, or several others that might be chosen, would be efficient. But since accident and decay compose the lot of all inanimate, as well as animated nature, let two or more sets of troughs, tubes, and wires be laid down; so that, whilst one may be undergoing repair, the other or others may be ready for use. Here a double advantage would be gained, since two or more communications might occasionally be going on at the same time, upon the same clocks and batteries.

3. The liability of the subterranean part of the apparatus to be injured by an enemy, or by mischievously disposed persons, has been vehemently objected.

More vehemently than rationally, I presume to hope (as is not unfrequently the case on these, as on many other sorts of occasions). If an enemy had occupation of all the roads which covered the wires, he could undoubtedly disconcert my electric signs without difficulty; but would those now in use escape? And this case relates only to invasions and civil wars: therefore let us have *smokers* enough to prevent invasions, and kings that love their subjects enough to prevent civil wars. To protect the apparatus from mischievously disposed persons, let the tubes be buried six feet below the surface of the middle of high roads, and let each tube take a different route to arrive at the same place. Could any number of rogues, then, open trenches six feet deep, in two or more different public high roads or streets, and get through two or more strong cast iron troughs, in a less space of time than forty minutes? for we shall presently see, that they would be detected before the expiration of that time. *If they could*, render their difficulties greater by cutting the trench deeper: and should they still succeed in breaking the communication by these means, hang them if you can catch them, damn them if you cannot, and mend it immediately in both cases. Should mischievous devils from the subterranean regions (*viz.* the cellars) attack my wire, condemn the *houses* belonging thereunto, which cannot *easily* escape detection by running away.

4. If, after all the preventive measures above mentioned have been adopted (and all those additional ones, which in practice would undoubtedly suggest themselves to an intelligent engineer), accidents or injuries of any kind should still occur, how to discover the seat of the disorder, the exact point or points which have sustained injury, and when it had taken place?

These are the most obvious, and not the least objectionable difficulties, of which I am at present aware. Let us see how nearly we can approach towards their dissolution.

Instead of placing only one such prover as K, Plate 3, at mid-distance between the two extreme stations, let a certain number of provers be placed at *about* equal distances from each other, accordingly as suitable situations for them may be found (such as post-offices in towns and villages, turnpike-gates, &c. &c.): and let a constant supply of electricity be furnished to the wire (by an easy method, which I shall presently describe). This, whilst the whole apparatus remains in good repair, will cause the pith ball electrometers of the provers to remain continually in a state of divergence, and show that it *is* so; and they will of course show when it is *not* so, by collapsing entirely, or by diverging less than usual; which last would be the most common case, I believe. Now (always calculating how far the expenditure of labour and pains spent upon the object are justly proportionable to the proposed advantage) let men be employed, who, living at *some* of the proving stations, may make it their business to watch the pith balls; and, on the moment that damage has been discovered, to ascertain, in the manner prescribed at page 11, whether the apparatus be defective on the right or on the left side of their provers. Let them then proceed on horseback (or send somebody else) in the direction of the defect, for the purpose of making experiments upon the neighbouring provers, and so on; always taking their course towards the defect, as before*.

To put a simple case: we will imagine twenty proving stations established between London and Brighton, or any distance of fifty miles, only four persons employed (but not exclusively) to keep watch over them, and each watchman to have the charge of five provers. It is evident, that (were he to dwell at the centre one of the five), in order to examine the two on each side of it, he would have to ride only four miles and eight-tenths; which journey even our twopenny post boy could perform in something less than forty minutes; and he would

* Any sorry little twopenny post *cove* might take a canter on his Rozinantuolo, and, on his arrival at a prover, perform the operation on it in less time than I have employed to describe the manner of its performance.

discover, that the defect rested somewhere between two of the provers, a distance of two miles and four-tenths. Let him report his discovery accordingly to the engineer, who may open the trench and the trough, at mid-distance of this two miles and four-tenths, make an experiment upon the wire itself, similar to that of the provers, and when he has discovered which half of the two miles and four-tenths is defective, operate upon that half in the same way. Thus proceeding continually, he must arrive, after ten bisections, within about three yards of the defect. Would two days be a shorter space of time than would be required to discover and repair the injury completely?

When my wire has to cross a bridge, it must be protected by a strong outward casing of wood, which should leave an interval between its interior surface and the iron trough; and a proving, or a watch station, should be established near the bridge.

A disorder occasioned by decay of any sort, would most probably arrive at its height gradually, and thus afford abundant time for reparation before it had rendered the wire wholly unserviceable; and an injury inflicted by mischievously disposed persons would *perhaps* be discovered by mere inspection of the road or street; for if they could open the trench and the cast iron trough in forty minutes, could they close it again in such manner as to make it impossible to *see* where it had been opened *immediately afterwards?*

5. We come now to the difficulties (if indeed they can be called such) of keeping up a sufficient and constant supply of electricity. As to sufficiency (I have no dread of the charge of vanity in borrowing a boast from the great mechanic), give me *materiel* enough, and I will electrify the world. The Haarlem machine would probably *in time* electrify, sufficiently for our purpose, a wire circumscribing the half of England: but we want to *save* time; therefore let us have a small steam engine, to work a sufficient number of plates to charge batteries or reservoirs of such capacity as will charge the wire *as suddenly* as it may be discharged when the telegraph is at work; and when it is *not* at work, let the machine be still kept in gentle motion, to supply the loss of electricity by default of insulation; which default, perhaps, could not be avoided, because (be the atmosphere ever so dry, and the glass insulators ever so perfect) conductors are, I believe, robbed of their electricity by the same three processes by which Sir Humphrey Davy and Mr. Leslie say, that bodies are robbed of their sensible heat, *viz.* by radiation, by conduction, and by the motion of the particles of air*.

* Mr. Singer, in his "Elements of Electricity," p. 283, seems to have misunderstood me in regard to the imperfection of his mode of insulation, adopted in some experiments

6. There may still exist in the mind of the electrician one plausible ground of objection, of which I am aware, beside those which have not yet suggested themselves, or have not been mentioned by others: Had the lateral explosion no share in the phenomena of the pair of gas pistols?

No, it had not: for when the jar was insulated, when the wire (seen in Plate 1 to connect the ball of the left hand pistol with the termination of the circuit of the eight miles of wire, suspended between the frames) was *detached* from that ball, and *attached* to the outer coating of the jar, when the same ball was connected with the earth, when the pistols were charged as before, when the jar was charged to exactly the same intensity as before, or a lower intensity, and when the discharge was made as before,—*then* the result invariably was, that only one pistol was fired. I could succeed in firing them both, *only*

on atmospheric electricity. It was the imperfect insulation of the spaces of air comprised between the mouths of the tubes and the wire, distances of half an inch, and not that comprised between the wire and the earth, a distance of *fifteen* or *sixteen* feet, to which I *partly* attributed the defect. But my chief enemies were the *spiders*, who frequently obliged me to walk a quarter of a mile, through long wet grass, to destroy the webs which they had spun within the tubes, for they thus established a semiconducting medium between them and the wire. Another source of inconstancy and error arose from the moisture requiring as much time *to get out* of the series of tubes as it had taken *to get in*.

by charging the jar to an intensity at least double that of the former experiment, or by recurring to the former arrangements.

Again: when any-one stood upon an insulating stool, took the wire connected with the end of the circuit in one hand, and touched the ball of the pistol with the other, so as to allow the charge to pass through his body, he could never distinguish the smallest interval of time between the shock which he felt and the simultaneous explosions which he heard; yet the shock was *greater* than that which he received from the same charge of the jar when it did not pass through the wire. The same thing, of course, occurred when he was not insulated.

Our first experiment completely failed, then, to impugn the orthodoxy of the worthy Doctor's school, whatever those of Signior Volta may have done. How far *it* and the subsequent experiments and observations may warrant the prosecution of further and more extensive undertakings, for the purpose of *endeavouring* to establish ELECTRIC TELEGRAPHS, remains for the consideration of those whom it may concern, and who may deem the subject worthy of consideration*.

* Lord Melville was obliging enough, in reply to my application to him, to request Mr. Hay "*to see me on the subject of my discovery;*" but before the nature of it had been yet known, except to the late Lord Henniker, Dr. Rees,

May I presume to hope, that the most material obstacles have been fairly stated and discussed? The advantages proposed are, in the first place, *Constancy:* it is easily perceptible, that neither climate, season, time of day, or any of the circumstances which most impede the utility of telegraphs now in use, could have any influence upon this. *Secrecy* is a second proposed advantage; and *Dispatch* a third.

Although "volenti nihil difficile" has been chosen as an *appropriate* motto for a humble projector, is it not equally applicable to a George the Fourth?

P.S. It need scarcely be mentioned, that a dictionary of the above-mentioned kind may be extended at pleasure. I believe, that a similar contrivance has since been rewarded by a medal by the Society of Arts.

Mr. Brande, and a few friends, I received an intimation from Mr. Barrow, to the effect, "*that telegraphs of any kind were then wholly unnecessary, and that no other than the one then in use would be adopted.*" I felt very little disappointment, and not a shadow of resentment on the occasion, because every one knows, that telegraphs have long been great bores at the Admiralty. Should they *again* become *necessary*, however, perhaps electricity and electricians may be indulged by his Lordship and Mr. Barrow with an opportunity of *proving* what they are capable of in this way. I claim no indulgence for mere chimeras and chimera framers, and I hope to escape the fate of being ranked in that unenviable class.

for my good intentions in attempting to devise a very simple remedy (be it but a partial one) for a long lamented grievance; but I assure myself of the *electrical patient's* approbation.

Reflecting, that the cause of all the mischief arose, not from the actual temperature of the cylinder, the plate, or the glass insulators of the apparatus, but from their state of temperature relatively to the ambient air (for the reason which I have assigned in describing the atmospheric electrometer), and having learned, by tedious experience, that, so long as a *hot* cloth was held by the hand under the cylinder, so as to form another rubber, its power was enormously increased, and that this additional power lasted only whilst it continued *warm*, I devised two expedients, which in a great measure had the required effect, and will be easily understood by the following description of Plate 5.

A is a cylinder of blue glass, almost a quarter of an inch thick; BB are its supports of wood; C a box, or case, forming the base of the machine; DD is a copper pipe, the supporter of a half cylinder E, also of copper, and hollow, into which the support, D, opens, and which carries the cushion and rubber; F is a very small spirit lamp, the burner of which is formed by only one thread of cotton, and it is placed immediately beneath the mouth of the copper pipe, DD. This was the first expedient.

The second scarcely requires description. It merely consisted in placing the prime conductor, G, upon a glass support, H, formed precisely in the same manner, and secured in its place in the same manner as the pillar of my atmospheric electrometer (see page 30), and another small spirit lamp was placed immediately beneath *its* orifice.

A cylinder machine, thus constructed, of half the dimensions of one made upon the usual plan, is very effective, and, what is better, *always* effective. It is *most* convenient for medical purposes; and, if it be required to add the advantage of portability, let the box or case, C, be enlarged sufficiently to contain a drawer, which drawer may receive the dismounted cylinder, the rubber with its support, and all other necessary apparatus. The two supports, BB, of the cylinder, being provided with hinges, may be shut down upon the top of the box, and thus the whole reduced to convenient dimensions for any mode of conveyance.

The same *warming pan* principle may, of course, be easily applied to plate machines. Heat probably assists the excitement by promoting the oxidation of the amalgam, which is a discovery of the learned and ingenious Dr. Wollaston, I believe.

DESCRIPTION

OF

A NEW MODE

OF

ELECTRICAL INSULATION*,

and of some

EXPERIMENTS ON VESUVIUS, &c.

ALTHOUGH atmospheric electricity, from the period of its first discovery, has always been esteemed an important subject of inquiry, and has been the object of much diligent and valuable investigation, it is regretted, that a register of careful observations upon it do not accompany those made in various places, on the pressure, temperature, humidity, &c. of the atmosphere.

The difficulty, if not the impossibility of constructing a correct atmospherical electrometer, and the great inconvenience attendant on the use

* The electrometer here described differs very little from that which I have described in the Quarterly Journal, vol. ii, p. 249, which is perhaps more convenient for portability.

of the least objectionable kind of instrument (which, perhaps, is the " exploring wire" of Father Beccaria), are no doubt the causes of this omission.

The *known* difficulty seems to be that of preserving the glass supports, or insulators of the wires or rods, in such a state, that, as far as they are concerned in insulating them, they may do so uniformly; but no attempts to effect this object have hitherto succeeded, probably because the deposition of moisture upon them, from the surrounding air, has not been totally prevented. Mr. Read imagined, that, if his insulators could be constantly kept in " due temperature," his rod would be always electrified; but he feared, that, as this could only be accomplished with the aid of common fire, it would be very difficult, in so large an apparatus, to apply neither too much nor too little. In the apparatus described below, the temperature of the insulator may be (constantly, or whenever an observation is made) raised a little higher than the surrounding air; and consequently it seems reasonable to conclude, that the deposition of moisture on its surface is extremely small, and unappreciable by its conducting power.

That the difficulty is thus obviated, in a great measure at least, is evident, by comparing it with those kinds of insulators which are not warmed;

but it would be going too far to say, that glass, or any other substance, whose temperature is raised ever so little, is not thereby rendered more conducting. One or two other slight improvements on atmospheric electrometers are attempted, and the advantages of several different kinds are combined, so as to render it convenient and applicable to general use. But Mr. Ronayne's simple method, wherein no insulation is required, would no doubt be by far the most satisfactory, if it could be adapted to *small intensities*.

A (Plate 6, Fig. 1) is a glass pillar, which perforates a circular piece of hard box wood B, and also the larger circular piece C; the sides of these perforations are lined with thick leather; DDD are bolts which pass through B and C, provided with nuts, by means of which, and in consequence of the conical figure of the pillar, it is firmly secured in its position, without danger of being broken.

That part of the support is hollow which is marked with dotted lines; the thickness of glass is about a quarter of an inch at the opening, and the upper part is covered with sealing wax; beneath the pillar is a small spirit lamp E (protected from wind by a glass chimney), in which only one small thread of cotton is used, to convey spirit to the flame; the upper end of the pillar is furnished with a strong brass socket, to which is attached the ball F; this is pierced to receive the lower joint of a long bamboo fishing rod, G, which has been gilt; it inclines at an angle of about 45°. The strong wire, H, carries the upper part of a Volta's straw electrometer I, or any other electrometer which may be most convenient for the required species of observations. On the opposite side of the pillar a similar wire is employed for similar purposes.

I is one of Volta's electrometers, modified only by the piece, which supports the straws, being perfectly *detached* from the glass bottle—by that piece being *attached* to the arm H when the instrument is in use, and by its carrying a cone, or funnel, K, similar to that used by Saussaure in his electrometer; the mouth of the bottle is about half an inch in diameter; the piece carrying the funnel and pair of straws is suspended from the arm H in such manner, that, by turning the ball L, all vibratory motion is prevented, and it can be removed at pleasure, *instantly*, in order to substitute a gold leaf electrometer, which may occupy the place of the above, upon the little table M, with a sliding support.

A pair of very small balls, turned from box wood charcoal, are suspended from the arm N by the finest silver wire, six inches long; or, in lieu of them, the electrometer, O, which serves to note the higher intensities, such as of rain, heavy clouds, hail, snow, &c., which being too great to be measured by those more delicate electrometers, they can be easily removed; and, if it be required, two other arms, at right angles with H and N, may be affixed to the socket carrying the two larger-sized electrometers of Volta.

The apparatus, as represented in the figure, being screwed upon a table, in the highest room of a house, and the fishing rod thrust out of the window, is in a proper state for examining the usual atmospheric electricity of serene weather. If it be used in the open air, the rod can be placed vertically in the socket, and the ball F dispensed with; and, if a *wire* be used, two such insulators and rods may be employed to sustain it.

Signior Beccaria took particular notice of the *frequency* of electric signs in his wire, or the rates of time at which fresh signs arose after he had touched it, which, as they are proportionable to the degree of atmospheric moisture, he very justly considered as deserving peculiar attention. It is principally for this purpose that fine silver wire and hard charcoal balls are perhaps preferable materials for electrometers, to threads of any kind, and pith or cork balls, or even straws, since these, by becoming very dry sometimes, and then not conducting equally well, diverge more slowly than they ought, and are consequently equivocal indicators of this property of frequency; and the practice, which has been recommended by some electricians, of moistening them with salt water, in order to render them better conductors, not only renders them objectionable, on account of their liability to twist, bend, and shorten, but because their weight is not immaterially varied by this process. Pith or cork balls, when not new, are very apt to stick*.

Fig. 2 is a convenient instrument to be employed in the construction of electrometers of silver wire and charcoal balls, which is, as Mr. Cavallo very justly observes of those made with cork balls and silver wire, very difficult in the usual way. ABCD is a bow of steel wire, having a hook at each extremity. After the ball has been threaded on the silver wire, and rings formed at each end, it is *very gently* stretched in this bow, by passing the hooks through the rings and shoving it forward, with the thumb placed against the end of the tongue, near the handle, which tongue is thus made to open wider, by pressing the screw E on each side; then the screw is turned a little further into the piece F, in order to secure it firmly in its place. The fine wire is now carefully laid upon a piece of iron, heated a little below redness, which renders it perfectly straight, and it may be removed from the bow, and hung upon one of the rings of the piece of brass, Fig. 3.

* This circumstance may be worth attending to in experiments upon the electric columns of M. De Luc, perhaps.

Fig. 4 shows a manner in which gold leaf electrometers are rather more conveniently and correctly formed than by the ordinary method*.

With an apparatus of the above kind, I made some experiments, in 1819, on Vesuvius †, at the time of *moderate* eruptions, and at Palermo, at the time of a Sirocco wind, the results of which only I shall trouble the reader with.

* MM. les Redacteurs des Annales de Chimie, in honouring this contrivance with a notice in their valuable journal, tom. iv, p. 104, say, that, " au lieu d'echauffer la colonne de verre qui isole l'electrometre, il est plus simple de placer l'instrument dans une petite cloche ou cage de verre, dont on tient l'air dessechè, eu y laissant de la chaux vive, du clorure de calcium, de la potasse caustique, ou de l'acide sulphurique concentré. L'instrument doit porter une tighe metallique, qui traverse la cage sans la toucher, et qui laisse, autour d'elle, un jou *d'un a trois millemetres*" — and that they had often employed this means advantageously. Now I adopted the use of *heat*, after I had found hygrometric substances both *imperfect* and *inconvenient;* imperfect, because it was necessary to render the above-mentioned interval *so small*, that in a thunder storm, &c. a discharge took place through it, and I could never be certain that the insulating power of the glass was *uniform* in all states of the atmosphere: inconvenient, because it was necessary to wait a considerable length of time before they had produced the desired effect (indeed, so long, that the phenomena required to be observed had perhaps vanished); and if an hygrometric substance were kept constantly enclosed in the bell, it soon became *saturated*, and no longer capable of drying either air or insulator.

† See Quarterly Journal, vol. xiv, page 333.

The rod of the electrometer was placed perpendicularly on the highest pinnacle of the mountain, on the north side of the great crater, and at about five hundred yards (across a ravine) distant from it. It was fourteen feet high, and the insulation was very good, for, when electrified *artificially* to a much higher degree than it ever became *naturally*, it retained its electricity for full five minutes, without any sensible diminution. A pair of straws, made exactly according to Volta's standard, of the third, or smallest size, was attached to the rod. The *results* were as follow, *viz.*

The electricity was *constantly* positive.

The intensity increased as the sun rose (as usual) except when influenced by explosions of the volcano, &c. The variations of intensity occurred very frequently. The difference between the highest and lowest degree of intensity amounted nearly to one-third of the mean intensity.

These variations of tension sometimes accompanied changes of wind, which often occurred six or seven times in the course of half an hour. The wind most frequently blew from the south, but often *suddenly* changed to north-east and north.

Sometimes they immediately *followed* explosions from the craters, and sometimes did not seem to be at all influenced by them, or by a wind which came directly from the crater at the time of an explosion.

Sometimes they were evidently produced by the approach of vapour from an aqueous fumerole, and then the tension was always increased: sometimes the tension was diminished very rapidly after an explosion, and sometimes increased as rapidly.

The black fumes from the old crater evidently diminished the tension much more frequently than the white, and very rarely increased it.

Finally: it seemed sometimes utterly impossible to discover any coincident occurrence, which could produce these changes; so that perhaps it may be in vain, without further experiments, to attempt any rational explanation of the phenomena. Would it be fair to suppose, that the black fumes may be in a negative state, and that the white fumes, consisting principally of aqueous vapour, sulphuric and muriatic acids, and sulphur, may, when these vapours become condensed (as in Volta's artificial experiment), and when the sulphur sublimes in the air, be brought into a positive state; and that these two states of the black and white fumes may sometimes act separately upon the electrometer, or sometimes wholly and sometimes partially neutralize each other, either by induction or position, or by a discharge from one to the other? In *violent* eruptions, no doubt, the frequent flashes of lightning, which are seen to take place amongst the clouds of smoke and vapours proceeding from the crater, are occasioned by such discharges.

The thermometer in the shade on the mountain, on the 4th of July, at eight o'clock A.M., stood at 76°; and in the sun, with a blackened ball, at 83°. At the same hour at Naples it stood at 78° in the shade, and at 100° in the sun, with a black ball*.

The experiments at Palermo, in the time of a Sirocco, were made on the roof of Page's Hotel and on Mount Pelegrino. As the former were the most numerous and most satisfactory, I shall give an example of them, selected from a

* The magnetic needle on the mountain never exhibited, with me, any such extraordinary signs of oscillation as it appears to have done with Spallanzani; neither could M. Gimbernat, councillor of state to the king of Prussia (who witnessed some of my experiments on Vesuvius), ever perceive any such effects, although he frequently tried the experiment. M. Gimbernat is the gentleman to whom visitors to Vesuvius are so much indebted for the luxury of a little fine pure water in this region of heat, thirst, and fatigue, by the establishment of apparatus for condensing the vapour of the aqueous *fumeroli*. In some of the waters so condensed he has discovered *animal* matter.

series, which bore a strong analogy to each other.

	TIME. H. M.		ELECTROMETER.		
July 30.	1 0	A.M.	10°	*Pos.* ... Sky serene,	no wind.
	1 15	....	12°	.... Id.	.... Id.
	2 0	....	7°	.... Id.	... A slight breeze.
	3 0	....	6°	.... Id.	.... No wind.
	3 15	....	6°	.... Id.	.... Id.
	3 45	....	6°	.... Id.	.... Id.
	4 0	....	5°	.... Id.	.... Id.
	4 15	....	7°	.... Id.	.... Id.
	4 45	....	6°	.... Id.	.... Id.
	5 0	....	6½°	.... Id.	.... Id.
	5 30	....	5°	.... Id.	.... Id.
	5 45	....	5°	.... Id.	... A slight breeze.
	6 0	....	6°	.... Id.	.... Id.
	6 15	....	6°	.... Id.	.... Id.
	7 15	....	8°	.... Id.	.... No wind.
	7 45	....	8°	.... Id.	.... Id.
	8 0	....	9°	.... Id.	...... Slight breeze.
	9 15	....	18°	.... Id.	.... Id.
	9 30	....	17°	.... Id.	.... Id.
	M.	....	16°	.... Id.	.... Id.
	0 30	P.M...	16°	.... Id.	.... Id.
	0 45	....	15½°	.... Id.	.... Id.
	1 0	....	16°	.... Id.	.... Id.
	1 15	....	16°	.... Distant clouds.	Id.
	2 0	....	20°	.... Id.	
	3 15	....	21°	.... Sky serene.	Id.
	3 30	....	20°	.... Id.	.... Id.
	3 45	....	20°	.... Id.	.... Id.
	4 0	....	18°	.... Id.	.... Id.
	4 15	....	13½°	.... Id.	.... Id.

	TIME. H. M.		ELECTROMETER.		
July 30.	4 45	P.M.	12½° *Pos*...	Sky serene,	Slight breeze.
	7 30	....	12°	Id.	Id.
	8 0	....	14°	Id.	Id.
	10 0	....	11°	Id.	Id.
	10 15	....	15°	Id.	Id.

The electrometer employed was Volta's smallest pair of straws. The thermometer at twelve o'clock in the day stood at 92°, and at fifteen minutes past one A.M. of the next day at 84°, between which two points it fell gradually. The barometer stood fixed the whole time at 27.64*.

* The Sirocco, at Girgenti (the ancient Agrigentum), and on all the south side of Sicily, is highly charged with vapour; but at Palermo, after having passed over the arid mountains, it is extremely dry. I was once at a conversazione at Palermo when a Sirocco came on, and the window opened for air, by an English gentleman, who was not aware of the necessary precaution of shutting it out. The instant effect was a loud cracking noise, occasioned by the shrinking and tearing of the paper with which the room happened to be hung.

On another occasion, at Palermo, I exposed half an ounce of water to its action in a plate, which evaporated in twelve minutes, yet the temperature was only 84°. The degree of dryness, by De Luc's hygrometer, 78°. The plate was placed at a window, where the draught of air was very inconsiderable, the door and other windows being all well closed.

At Naples, the Sirocco, having passed over the sea, arrives highly charged with vapour, where it produces, even in winter, that extreme lassitude, which the Neapolitans say makes BAD BOOKS.

In its progress up the Adriatic, *when it begins to blow after a Maestro, or north-west wind*, it creates the storms and

It will be seen, that in the above table of observations the electric phenomena of the Sirocco were diametrically opposed to those of the ordinary state of the atmosphere in serene weather, since the electric tension is observed to increase almost progressively from sun-rise until the hottest part of the day, *viz.* about three o'clock, when it again gradually declined until sun-set.

But if these observations differ so widely from those made in this country, or in the ordinary state of the atmosphere in Sicily, how much more do those which were made by the Arctic expedition in the year 1819-20? It is said, that *no* electric signs are in those regions sensible "in the summer months, when the clouds become more dense and frequent, and even when a slight shower of *rain* falls." This is to me the most extraordinary of all the facts, with which the researches of the brave and indefatigable Cap-

Trombi, or water-spouts, which so frequently occur in that dangerous sea. The hot Sirocco, after traversing a large expanse of water, has, probably by the solvent power which it derives from its heat, taken up a great deal of vapour, which, meeting with the colder Maestro, becomes suddenly condensed, and creates these thunder storms and *Trombi*.

In Egypt and Arabia (where it is called the Simoun), as every one knows, it is a blast from Hell, capable of annihilating, by its extraordinary heat and dryness, all animal and vegetable life.

tain Parry and his most meritorious associates have enriched the scientific world. But I can scarcely avoid suspecting the apparatus employed of *some* degree of imperfection. That no signs should occur in the *winter* months, when the temperature of the air is but little changed, and consequently its state of humidity little varied, and when its very low temperature allows it to hold but little water in solution, does not seem so extraordinary.

May I be excused for stating in this place a few queries, &c. relating to atmospheric electricity, which neither inclination prompts nor circumstances allow me to *attempt* the investigation of.

1. If invisible vapour absorbs electricity in proportion to its rarity or elasticity, will not this circumstance account for the *ordinary* gradual decline of electric signs towards the middle of the day? For when the earth is heated in the morning by the rising sun, the vapour which exhales is perhaps partially condensed again by the cooler air in the neighbourhood of our rods, &c. If this partial condensation did not occur, the vapour would not be more disposed to part with its latent electricity than with its latent heat. If the vapour had not an affinity for electricity, it would not have taken it; and if it took any, we may reasonably suppose that it took just as

much as it required to be saturated. But when the sun has remained a sufficient length of time above the horizon to rarefy the vapour again, and the exhalations from the earth have ceased or diminished, the partial condensation should be less, or it should cease also; and consequently the disposition of the vapour to part with its electricity should be less, or should cease, or it should even tend to rob the wire of electricity. As the sun sets, these effects of course should be reversed. But here questions arise as to the changes of temperature that the wire or rod may experience by radiation, &c., which may happen in the manner pointed out by Dr. Wells, in his excellent work on Dew; and whether the *kind* of metal composing the wire or rod has any influence upon the signs.

2. *The conducting power of air*, relatively to its temperature, to its humidity, and to its rarity, are all circumstances which should be taken into consideration, and their influences examined, if we would arrive at a further knowledge than we at present possess of this intricate subject. Certain it is, that a cloud is a much better conductor than invisible vapour; but what relation the conducting power of the latter, mixed with common air, has with perfectly dry air, is a question still to be determined. Is not the cause of M. Saussure's not having been able to

obtain electric signs from the vapour of water, which he exposed to a moderate heat, in boilers, &c., attributable to the conducting power of the cloud, which *must* have *gradually* risen from them, and *may* have restored the electricity of his electrometer to them as fast as it was carried to it by its connection with both sheet and electrometer*? And may not the circumstance of his having obtained signs from vapour raised by projecting *very small quantities* of water upon heated iron, &c., be referable to the separation of the *little* cloud from that iron, &c., by an interval of drier insulating air? I apprehend, that some of the singular effects produced by the small clouds (raised by the sun from the snow) upon a pointed conductor on the Mole, which he observed, owed their origin to some such cause†.

3. It may seem rather difficult to admit, that the ambient air of conductors, that which we breathe, and in which our electrometers are immerged, should naturally possess a perpetually varying state of electricity distinct from its *latent, or chemical electricity* (if I may be allowed the expression), yet the hypothesis is plausibly supported by both argument and experiment; and, if it be

* Saussure, Voyage des Alpes, tom. i, p. 242.

† Idem, tom. i, p. 241.

true, how greatly must it influence all our electrometrical deductions. Whilst I plead guilty to a certain degree of scepticism on this point, I also acknowledge, that there appears to me to exist no good reason why it should *not* be so. For, by numerous experiments, it has been often proved, that, when a bottle electrometer, whose sides have no conducting connection with the earth, has remained immerged for a certain length of time in a lower stratum of the atmosphere, the electric states of the included and excluded air are placed in equilibrio, and no signs are exhibited, even should that lower stratum be shown (by another electrometer, whose sides *have* connection with the earth) to be highly charged with electricity.

It is only the difference of the electric states of two different portions of air that can be measured by *such* an instrument.

In whatever manner an electrometer becomes charged, it only *exhibits signs* whilst electricity is either flowing, or tending to flow, either from it, or to it, in order to restore an equilibrium. When, therefore, it has been suddenly raised from a lower stratum to a higher, it exhibits signs which indicate the difference between the electric state of that higher stratum, and that of the air contained in the bottle and the sides of the bottle, *which it has brought with it* from the lower stratum; and if it remains in the higher stratum until the signs cease, we *only* know that the electric equilibrium is restored between the included and excluded atmospheres and the sides of the bottle. When it has been suddenly returned to the lower stratum, it again exhibits signs (for the same reason), which again cease when the equilibrium is again restored*.

Now suppose that observations were required to be made upon a *third* stratum of the atmosphere, with an electrometer furnished with a long rod, which should extend from the second to the third, is it not too evident to require more words, that, until we know the difference between this second stratum and the first, and between the first and the earth, we can *never* accomplish our purpose?

We only complicate the affair, by establishing a conducting communication between the sides of the electrometer and the earth; for an electrometer owes but a part of its divergence, or signs (and sometimes but a small part), to the attraction of its *uninsulated* sides, as may be readily proved, by bringing the hands within certain distances of the balls of a common Canton's electrometer which has been previously electrified.

It *may* be probable, that, either by position or induction of the higher strata of the atmosphere,

* The best experiments of this kind are those of Mr. Erkman. See Gilbert's Annalen, tom. xv, p. 308.

by hygrometric or chemical changes, or by unknown causes, even the air which we breathe, our own persons, and all the *conductors*, partial conductors, and even non-conductors (if such exist) which surround us, may be maintained in a *variable* state of electricity. The experiments of some French savans, a few years ago, on the so called spontaneous electricity of various surfaces, and those of Mr. Bennett and Mr. Read, &c., with the revolving doubler, seem to warrant this conclusion. And who, until the fact be either refuted or established, will attempt the construction of an atmospheric electrometer?

"Signior Beccaria conjectured, that, since a sudden stroke of lightning gives polarity to magnets, a continual flow of the fluid from north to south may be the original cause of magnetism in general*." A fine idea! Have Ritter's experiments on secondary Voltaic piles any relation with the subject? Or the new electrico-magnetic experiment of Dr. Œrsted, Sir H. Davy, and others? Any such continual flow may take place, and may even suffer variations in intensity, &c., yet we and our imperfect instruments remain as little sensible to what is passing, as our forefathers were to the motions of the earth, or the pressure of the atmosphere, &c. &c.

* Beccaria Lettere dell' Elettricismo, p. 263.

the electricity of serene weather, and of dew, are not, however, less interesting, or less deserving attention, and they equally require an instrument to note them, but for the opposite reason, *viz.* their tediousness*. Fig. 1, Plate 7, is an electrograph, which may be applied to either purpose.

AA is a box, containing a strong timepiece, placed in a horizontal position, and receiving motion from the weight B. CC is a circular plate of baked mahogany wood, eight inches in diameter, having a perforation, D, of two inches and a half diameter. The circumference of this plate, and that also of the perforation, are provided with edges, or rims, and the outer broad rim is divided off, and marked with hours and minutes, in the manner of a common clock. The space between the two edges is nearly filled with cement, composed of resin, bees' wax, and lamp-

* I have received much amusement from a visit to Mount Gazegna, near Mondovi, the scene of Beccaria's beautiful and original experiments on the electricity of serene weather, dew, &c., and rather envied him *some* of the pleasures of his *serene* occupation ; but who, that could have *calculated* the tedium of the task, would ever have undertaken them ? If it be true, that " fatigue generally begins and is always increased, by calculating in a minute the exertion of hours" (which I firmly believe it is), may not the argument be as aptly applied to pleasure ? The cherry tree no longer remains.

black, and this part of the apparatus can be detached at will from the box. EF is a glass tube, furnished with brass caps (and covered both inside and out with hard cement), the lower end of which screws upon the dial plate of the timepiece, and the upper end carries a small cylinder, or sheave, *g*. *Within this tube*, EF, a stem of glass is fixed by its lower end on the minute arbor of the timepiece, and a pivot, attached to its upper end, passes through the cap F and the cylinder *g*. This pivot carries the iron ball and cup, *h*, into which is screwed a steel wire, *i*, and this carries the piece *k*, which may slide with a little friction upon it. The wire *l*, fixed into the piece *k*, terminates at its lower end in a hook, and another short wire, *m*, is furnished with a ring at one end, by which it is attached to the hook, and with a small gold bead at the other, which rests upon the resinous plate. Lastly, a fine thread, *n*, is also attached by one end to the piece *k*, and by the other to the cylinder *g*.

When the clock is in motion, and the apparatus disposed as is represented in the figure, it carries round the arm *k*, and of course causes the thread, *n*, to coil itself round the stationary cylinder *g*, the piece *k* to advance towards the ball *h*, and the gold bead, which *trails* upon the resinous plate, to describe a spiral thereon.

And when a communication is established between the little iron cup above *h* (which contains a globule of mercury, in order to secure perfect contact), and a wire connected with any species of atmospherical apparatus, the gold bead acts upon the resinous plate like Mr. Bennett's electric pen, *i. e.* it electrifies it in such manner, that when the plate is removed from the clock, and powdered with pounded resin, or even common *dry* hair powder, the line of the spiral exhibits configurations, which vary in form and in breadth according to the kind and intensity of electricity which the bead has communicated to it; and, by reference to the divisions on the circumference of the resinous plate, it is easy to discover the exact periods at which these occurrences took place. In short, a *comparative picture* of all the phenomena of atmospheric electricity, during the absence of the observer, is thus procured.

If the instrument be used for noting the phenomena of serene weather, dew, &c., the hour arbor is generally preferable; if for those of a thunder storm, hard shower of rain, or hail, or snow, the minute arbor; but I have sometimes found, that a more rapid motion is required than either, which may of course be obtained by the addition of a third arbor, &c.; and the glass tube, EF, with all its appurtenances, can accordingly be easily transferred from any one arbor to another, and the plate adjusted to a new centre. It is also necessary sometimes to employ a cylinder, either larger or smaller than *g*. In the first case, when the more violent and more transient phenomena are to be noted; and, in the second, when a delineation of a longer period is required to be executed by the instrument; for it is evident, that, in proportion to the diameter of the cylinder *g*, will be the proportions of the volute upon the resinous plate; and that the comparatively short duration of a storm, or shower, &c., which draws a larger figure, must require a space of greater breadth, as well as length, than the other, in order to avoid confusion; the cylinder *g* can therefore be removed, and others substituted in its place.

One advantage, which I have derived from this contrivance over a cylindric electrograph, is, the power of conveniently bringing the resin into a fit state to receive the electrical drawing, the only *certain* method of doing which is to pass a heated plate of iron over it, at two or three inches distance, in order to melt it *partially* (so perfectly does it retain the figure, and so difficult is it to destroy that figure without communicating a new one by the ordinary methods); which process of heating it is almost impossible to perform upon any other surface than a plane, so as to preserve a fine even surface.

But the principal advantage over both the cylindric and plane electrograph, *proposed* by Magellan, is that derived from a comparative and comprehensive view of the daily periodical returns of the phenomena: those, for instance, of the morning and evening electricity, which Beccaria found to bear a striking relation with the periods of sun-rise and sun-set, and which he accounted for by the sun's action upon the vapours which were exhaled from the earth. Magellan's plate electrograph would be very cumbersome and inconvenient for such observations.

Would not the above be also a proper instrument for observations on that most extraordinary tendency which thunderstorms have to reappear, many days successively, about the same hour; and, what is more, at the precise spot where they had appeared at first. "It is necessary to inhabit," says Sig. Volta, the learned and sagacious discoverer of this new phenomenon, "a mountainous country, and particularly the neighbourhood of lakes, such as Como, the precincts of Lario, Verbano, Varese, Lugano, Lecco, and the whole mountain of Bianza, Bergamo, &c., in order to be convinced of such periods and fixations (so to speak) of thunderstorms at this or that valley, or opening of a mountain, which last until some wind, or remarkable change in the atmosphere, shall occur to destroy them." Sig. Volta refers the cause of the phenomenon to a modification in the ambient air, produced by the thunderstorm of the preceding day*.

* Lettere al Configliachi Giornale di Fisica, Lettera Prima.

DESCRIPTION

OF

A PENDULUM DOUBLER.

WISHING to convince *some* of those, with whom seeing only is believing, that it was possible to keep my telegraphic wire constantly electrified with a very small source of electricity, I converted the *bob* of a pendulum, belonging to one of the clocks, to the purpose of a centre plate of a Bennett's or Nicholson's doubler, and found the instrument, so modified, convenient, not only for that purpose, but for all those kind of experiments which require a constant flow of *small quantities of electricity* to supply the loss occasioned by unavoidable defective insulation, either in the glass which is used, or in the surrounding atmosphere: such experiments, for instance, as those on vegetation and animal life.

Fig. 2, Plate 7, may give an idea of the method which was adopted. A and B are the two stationary plates, fixed upon glass supports; C is the bob, attached to the pendulum rod D by the glass insulator *e;* its form is a plano-convex lens, and the interior is filled with lead; *f* is a small cylinder, through which screws pass to connect it with C, and to adjust the plane of C exactly parallel to the plane of vibration; *g* is another insulator, carrying at its lower end the bow of wire, *h,* the left side extremity of which is situated nearly in the same perpendicular plane as the end of the wire *m,* and the right side is nearly in the same perpendicular plane as the end of *n; i* is a wire, fixed perpendicularly into C; and *k* another, fixed (perpendicularly to the plane of vibration) into the brass cap at the end of the pendulum rod; *l* is a wire, screwed into the upper edge of the plate B; *m* is a long wire, fixed into the lower edge of B, and approaching to within a small distance of A, where it is bent at a right angle, and then projects in a line perpendicular to the plane of vibration; *n* is another wire, fixed into the edge of A, and projecting in a similar direction (after it has also been bent); but the length of the *projecting part* of *m* is considerably less than that of *n,* in order that the *right side* of the bow, *h,* may pass the end of *m* without touching it; lastly, *o* is a wire, fixed perpendicularly into the base of the instrument.

Since everybody, who may have had so much patience and indulgence as to read thus far, may not happen to have seen the *revolving* doubler of Nicholson or Bennett described, I shall shortly state the mode of action by which this pendulum doubler produces, by the motion of a common clock, a constant flow of electricity. It is seen, that when the centre of the bob, C, arrives opposite to the centre of the plate A, the *insulated* bent wire, *h*, will touch at the same moment the ends of the wires *m* and *n*, and establish thereby a metallic communication between A and B; and that the wire *i*, by touching the wire *o* at the same moment also, will establish a communication between C and the earth. Now, by one of the laws of induction or compensation—*i. e.* that property which any *insulated* conducting surface possesses, when placed opposite to an *uninsulated* conducting surface, with a plate of air (or other semi-conducting or insulating body) interposed, of condensing on, or attracting to itself (within certain limits) the electricity which has been given to insulated conductors in communication with it, *and, being thus charged, of inducing a contrary state of electricity on the uninsulated opposed surface*—by this law of induction, I say, if a quantity of *positive* electricity, producing a tension = 1 degree in the electrometer, be given to either A or B, whilst the centres of A and C are opposite to each other, that quantity will be nearly all condensed on A, and C will have a tension = nearly 1 of *negative* electricity.

If C be now allowed to begin its vibration, the connection of A and B with each other will be instantly broken, as also that of C with the earth, and they will be all insulated, and all retaining the electric states which they possessed before the connections were broken (*i. e.* A will be positive nearly = 1; B negative nearly = 1; and C positive almost 0).

When C has arrived opposite to B, the *uninsulated* wire *k* will touch the wire *l*, and thus place B in connection with the earth; therefore C, by virtue of its negative charge, will induce a positive charge in it nearly = 1.

When C arrives a second time opposite to A, all the former connections will be re-established, and the charge of B will (by means of the wire *m*) be nearly all condensed on, and added to the original charge of A, making a tension nearly = 2 of positive electricity, which tension will induce a tension nearly = 2 of negative electricity on C.

When C arrives again opposite to A, the same process recurs as before; and when it returns a third time to A, the charge of B, being added to A, will produce a tension nearly = 4.

And so the charges in A and C would go on, nearly *doubling* at each vibration of the pendulum, until their tensions would arrive at such a point, as to cause a spark to pass between them.

But P is a Leyden jar, furnished with a Lane's discharging electrometer, *q;* a connection is established by means of a small chain between it and A, and the distance between the two balls, *r* and *s*, is considerably less than that between A and C, therefore the spark will be given to the jar; and a spark will be continued to be given at the completion of almost every second vibration, until it is charged almost as highly as A is capable of being charged, or the sparks will continually supply the loss of electricity by any defect of insulation, either of the jar, or of any conducting body in connection with its interior coating within certain limits. My plates and bob are four inches in diameter*.

It has been proposed to adopt large doublers instead of the common machines, for exciting electricity in large quantities, with a view to the saving of labour; and Mr. Read has hinted at his

* The contacts of the wires do not impede the velocity of the vibrations, because they are made small enough to act as springs of a required force; but the electric attractions of the plates and bob *do* tend to do so. The pendulum is suspended by two springs, placed one at each extremity of a cross piece, to which the rod is attached, for the purpose of preventing the bob from being drawn, by these attractions, out of its assigned plane of vibration as much as possible.

knowledge of a method to cause the centre plates of revolving doublers to recede from the others in proportion as the charges advance (a necessary condition, if large quantities of electricity are to be obtained); but he died without having made it public.

Could not a *fine screw* be cut upon the axis which carries the centre plate, passing through the common support of the other two, which should *partially* produce the effect desired? Or could not such a screw be made to act upon levers, &c., which should proportion its recession exactly to the advance of the charges? The affair would not be very difficult to a clever mechanist, perhaps*.

* The term electricity has been always used, as being the most convenient to explain more concisely my ideas, &c. in these and in all the other experiments: it is not intended to imply a belief in the actual existence of a fluid, or of two fluids, which produce the phenomena called electricity.

generally known, excited considerable interest. Neither of these communications, however, having, I believe, on account of their length, been printed in the Philosophical Transactions, he commenced a series of papers on the subject in Nicholson's Journal, in June, 1810, vol. xxvi.

His first column consisted of the usual combination, of zinc, silver, and cloth, deprived of all moisture, except what it got from the air. He then substituted all kinds of vegetable and animal substances in place of the cloth, but found at last common writing paper the best calculated for his purpose, and began with this apparatus a new and very long series of experiments.

Some time after this, his ingenious friend Dr. Lind proved, by dismounting a column, and drying the papers before a fire, that a certain quantity of water is necessary to its electro-motion. About the same time, too, the use of Dutch gilt paper instead of copper or silver plates occurred to him, and he obtained more powerful *electric* effects with a column, consisting of seventy-six groups thus constructed, than when wet cloth was used and the old arrangement.

Before he was informed of rolled zinc, he constructed a column of eight hundred groups, composed of tinned iron plate and Dutch gilt paper, with which, in the winter of 1808, he began some observations on the frequency of the strikings of the gold leaves of Bennet's electroscope against its sides, which he found to increase "from the morning until some time in the afternoon, and then went on diminishing till night, ;" but as the spring advanced there was "*a gradual diminution of the motion of the electroscope.*"

He attributed the variations in the electric phenomena of his column, in a great measure, to changes in the electric state of the ambient air, or to that of the surface of the earth itself, thinking, with Volta, that "the electric fluid tends to an equilibrium amongst all bodies, including the air; and that the proportional quantities of electric fluid, which is actually possessed by air, is the standard of plus and minus in the electroscope; and the proportional quantity, which is actually possessed by the ground, is the standard of plus and minus concerning the differences in the electric state of insulated bodies, compared with its own." According to this theory he supposed, that the ambient air gives electric fluid to the negative end of the column, which has proportionably less than itself, and takes fluid from the positive end, which has proportionably more.

As he advanced, finding great inconvenience in the occasional adhesions of the gold leaves to the sides of the electroscope, he determined to increase his number of groups, until they were capable of moving small metallic balls; and he made two "chaplets" of tin and Dutch gilt paper, strung upon silken threads, of one hundred and forty groups each, and of half an inch in diameter, which he enclosed in glass tubes; but finding their effect diminished by lying in the tube, he afterwards made use of larger tubes to receive them, furnished with metallic caps and screws, terminating in hooks, in order to keep their axes in the centre; then he suspended chaplets like garlands, consisting of zinc and Dutch gilt paper, from the ceiling by silken threads.

Thus far he had arrived on the 30th of May, 1808; but his paper on the electric column and aereal electroscope was sent to the Royal Society on the 7th of March, 1809, by which time it had been imitated by others.

In October, 1810, he published a description and figure of a column of six hundred groups of zinc and Dutch gilt paper, of seven-tenths of an inch in diameter, supported between three glass rods, and of some experiments made with it on the conducting power of various substances, the most curious of which is that upon a piece of deal, whose conducting power he found to vary very considerably, according as electricity was passed through it in the direction of its grain, or transversely.

In November, 1810, he mentions his success, in having obtained the motion of a small gold bead, suspended by a silken thread between two balls, each connected with the opposite extremity of his column, and refers to Mr. Forster's chime, published in Tilloch's Magazine, vol. xxxv, page 205, but he found the motion much too rapid and too irregular to allow the vibrations to be counted, which was his only object in adopting it; he therefore had recourse to a new contrivance, consisting in the suspension of the gold bead by a silver wire from the positive extremity, which was attracted by a ball connected with the negative extremity, as often as the tension of the positive end again arrived at a sufficient force, after the discharge which the former contact thus occasioned (by the connection of the two extremities) had taken place, and he found this apparatus to answer his purpose very well.

Whilst M. De Luc was still amused with these ingenious experiments, which had now become well known all over the continent I believe, I had, in the year 1814, the good fortune to make his acquaintance; and prompted, partly by the opportunity which it gave me of enjoying a little of the society and instructive correspondence of so warm hearted, justly celebrated, and truly amiable a man, and partly by the novelty of the subject, I undertook a few experiments, which were published in Tilloch's Magazine in June, September, and December of 1814, vol. xliv*.

* A column, formed by *stringing* the groups upon a strong silken thread, was suspended within a receiver to an arm of a delicate balance, in such manner, that it could be subjected to the action of an atmosphere of extreme humidity produced in the receiver, and the water absorbed by it *weighed;* yet the insulating support of the balance and electrometer, being situated at some distance from the receiver, remained quite out of the influence of the moisture.

The conclusions drawn from these experiments, were,

1. That the *quantity* of electricity (as measured by the *frequency* with which new signs arrive at a given degree of tension after they have been destroyed) increases in some certain ratio with the humidity of the column, and the tension, also within certain limits.

2. That, if the degree of humidity be carried beyond those limits, the tension decreases in some certain ratio with the increment of frequency or quantity.

3. That, since electricity, or the electric fluid, did not pass off more copiously in the one case than in the other, by the support of the balance and electrometer, it must have passed off by the water contained in the papers, which was the very cause of excitement or development, otherwise the degree of tension would have advanced proportionably with the quantity excited.

4. That, if the last conclusion be just, the great quantity, or frequency, and low tension of Voltaic batteries, excited by acids and salts, may be referable to a similar cause, i. e. *to the incapacity of the exciting fluid to insulate that high degree*

But I very soon grew tired of counting the number of strikings of the last mentioned pendulum, and so idleness (as *prolific* a mother of invention as necessity, I believe) instigated the contrivance of a little apparatus, of which I gave a drawing in March, 1815, vol. xlv, to adapt the vibrations of an inflexible pendulum to two indices, for the purpose of registering the number of vibrations which occurred in my absence; and I procured six excellent columns from Mr. Singer, who had improved the manner of making them very considerably, to give motion to it.

But it seemed impossible, by these, or any other means, to arrive at well-founded conclusions relative to the appropriation of the dry electric column to the purpose of an aereal electroscope; on the contrary, after some time had elapsed, and I had taken great pains to render my apparatus as perfect as I could, I was surprised at the constancy and regularity of its action, when it was not influenced by changes of temperature and humidity; and was thus led (though more convinced than many others who had paid attention to the subject, that the electric

of tension, which the quantity generated or developed by their chemical action would otherwise create.

5. That heat in all cases increases both the frequency or quantity, and the tension, except when it has dried the column beyond certain limits.

column was nothing more than a modification of Volta's pile) to attempt several methods of compensating the effect of heat upon it (one of which is described in vol. xlvi of Tilloch's Magazine), and of excluding that of moisture, in order to arrive at the construction of an electrical chronometer.

M. De Luc died on the 7th of November, 1817. I believe he was as good a man as his numerous works prove him to have been a great one; and the subject (in this country) almost died with him.

MM. Desormes and Hachette presented, in 1803, to the Institut a memoir, entitled, "Recherches sur la Pile electrique de Volta," which was not printed; but M. Hachette, in his "Programmes de Physique, &c.," published in 1809, mentions a species of pile, constructed with amadon, one of which kind, after a lapse of three years, was still capable of charging a condenser.

The dry pile of Il Sig. Professore Zamboni was, I believe, first described in his memoir, entitled, "Della Pila elettrica a Secco, &c." printed at Verona in 1812. It was then thus formed: on the back of some sheets of paper, covered with zinc, called *carta argentata*, a coat of olive oil was placed, or a weak solution of honey in water, or a saturated solution of sulphate of zinc; and on this, whilst still moist, black oxide of manganese was sprinkled (for Volta had long ago found this substance to possess great electromotive power). These sheets, thus prepared, were placed one upon another, and stamped into round disks with a punch, and these were placed with the coatings of zinc all directed the same way, in a varnished glass tube, the interval between them and the glass being occupied by flower of sulphur, rammed down hard, but the olive oil was afterwards dispensed with.

Sig. Zamboni, in the same work, describes a horizontal pendulum, consisting in a long needle, suspended in the manner of a magnetic needle, which vibrated between two balls, each connected with the opposite extremity of a column; and having afterwards preserved a pendulum in motion for six months, he thought the dry column capable of furnishing a permanent source of electricity.

In Schweiger's Journal, vol. xiii, p. 379, it is stated, that H. Ramis at Munich, and H. Streizig at Verona, had each constructed an electric clock, marking hours, minutes, and seconds, which received motion from M. Zamboni's columns*: and I believe some other similar attempts were made in Germany, all which were treated here with little consideration, because it was said, that the irregularity of the power would always offer an in-

* See Thomson's Annals, vol. ix, p. 5.

surmountable obstacle to its application as a measure of time.

At the end of 1814, or the beginning of 1815, Sig. Zamboni presented a pair of his piles, and a vertical vibrating needle, suspended a little above its centre of gravity, to the London Royal Society; and I believe that a species of clock-work, moved by his piles, already existed in Paris.

The apparatus in the apartments of the Royal Society continued in motion many months; and, after it had stopped, one of the most important phenomena of the dry column, which was not previously known *with us*, was discovered by a gentleman (who, I believe, seldom fails to glean some new fact from the most sterile field of inquiry which may chance to come within his range); *viz.* that it absorbs oxygen very copiously from the atmosphere, and that the continuity of its electro-motive power in some measure depends upon such absorption. A fact somewhat similar to this had been observed in the early stage of researches upon the Voltaic pile by Col. Haldane, whose experiments led him to conceive, that its action " depended essentially upon the combination of oxygen, which it derives from the atmosphere*."

* See Dr. Bostock's very useful and concise History of Galvanism, p. 31.

H. Gilbert, in his Annalen der Physick, vol. xxxi, p. 188, gives an extract from the Stutgard Morgen-blatter of the 23d September, 1815, of which the following is a translation:—

"Another eminent artist, the university mechanic Buzengeiger, of Tubingen, has laid before the Queen a contrivance, united with the electric pendulum, which forms a clock, provided with second, minute, and hour hands. The circular inch-thick clock case rests, perfectly insulated, on a strong glass support, and the electric pendulum hangs down from out of this case. The lower end, provided with a hollow silver ball, swings backwards and forwards between two metallic balls, which are fixed upon varnished glass tubes, standing on the base. The piles lie concealed in the box of the base; and the wires, which unite their poles with the balls, pass through each glass tube. The axis of the pendulum is within the clock-case; and both above and below the axis the pendulum-rod carries a moveable lever. The ends of both these levers lock, by the oscillations of the pendulum, into the teeth of a little ratchet-wheel, so as to shove it on, which afterwards moves the other very simple wheel-work.

There has been, indeed, an instrument, called an electrical pendulum clock, exhibited by Professor Ramis at Munich, to the Academy of Sciences there, *some* months earlier; he kept the construction of it, however, secret; and the mere idea of the possibility of such a clock, must force itself upon every one who sees the vibrating pendulum of Zamboni: Zamboni himself has, indeed, much earlier accomplished it. The proposal of a Munich paper, of giving to such an instrument the name of Ramis clock, in order to secure to Bavaria the right of the first invention, cannot, therefore, be extended to the clock of Mr. Buzengeiger.

This modest artist imagines nothing less, than that his work, in respect to the exact measure of time, can be compared with a good pendulum clock, or used as an astronomical chronometer; for hitherto there has been no success in destroying the influence, which variations in the temperature and conducting power of the atmosphere produce on the rapidity of the charge of the piles, and therefore on the motion of the pendulum."

I have not mentioned the many ingenious and indefatigable researches of Dr. Schübler, H. Heinrich, Dr. Jäger, Professor Pfaff, H. Schweiger, and Grindel and others (which were made in a very accurate and ingenious manner), because they were not directed towards the attainment of a more permanent source of electricity from the dry column, to the application of its electro-motion as a mechanical power, or to the discovery of its insufficiency for such a purpose; but I believe, that very many of those experiments have conspired with time to convince everybody, that no *perpetual* power can be thus obtained: in short, that the dry electric column (as it is called) is after all nothing more than a modification of the humid pile of Volta, and that it has *not* furnished us with that grand desideratum, an instrument to assist us in investigating the habitudes of the electric fluid or agency on the surface of the earth and the ambient air. With all possible deference to the most respectable authorities, I cannot avoid forming this conclusion.

Yet time and experience have *also* proved, that if the column be constructed with strict adherence to the principles and directions of M. De Luc and of Sig. Zamboni, its power will diminish *very slowly;* and the watchmakers tell me, that their main springs would, if not compensated by the action of their pendulums, exert their force as irregularly as I tell them that my electric pendulum acts: why therefore may *we* not have a compensating pendulum too—a *secondary* compensating pendulum?

These considerations induced me still to continue my attempts, at different intervals, to make an electrical clock; and in 1816 I sent to M. Dumotiez (the best instrument-maker in Paris) for some of Sig. Zamboni's most powerful and largest columns (for now they were made by many in Paris, I believe), and in the mean time I had a clock, with three hands, constructed, on the same plan as the first, carrying a pendulum which weighed four hundred and thirty grains; for experience as well as reason suggested the propriety of causing our electric pendulum to partake, as much as possible, of the nature of a common pendulum.

The four columns which I received were at first extremely powerful, so much so, that I procured the motion of a horizontal wheel, of the nature of Franklin's jack, with them. They were capable of yielding a spark about a fiftieth of an inch long, and my new clock went on gloriously: but in the course of two or three months they became *fatiguées*, as M. De la Matherie found his to do in 1815; and now not the slightest signs are perceptible. I therefore had recourse to some old columns, one inch and five-eighths square. I made a considerable addition to their number. I procured a square box, into which I poured cement to the height of about an inch and a half. I arranged the groups of silvered paper and zinc upon this cake of cement, leaving an interval of an inch and a half between their ends and the sides of the box, and between each other; and then I filled up the box with cement, taking care, however, first to unite them all properly, and to connect wires with the positive and negative extremities of the whole series, by passing them through glass tubes lodged perpendicularly in the cement. These wires communicated each with a flat insulated plate of brass, and the insulated pendulum was made to vibrate between these plates; but instead of striking them, it struck gold wires stretched by steel bows, something like that used by M. De Luc, fixed in such manner to the plates, that it could approach and be *attracted* by them before the contact with the wires occurred. It was not the *bob* of the pendulum which struck these bows, but the lower part of the brass rod, which, presenting a length of three inches to the plates of the same length, offered an attracting surface, the whole of which could approach within a twentieth of an inch of them before the discharge occurred. I need not say how much was gained by this arrangement. If it be true, that electric attraction increases in the inverse ratio of the squares of the distances, the force and rapidity of the pendulum, when it arrived at the end of its semi-vibrations, should be very great; and so it actually was; and the bows *reacted* proportionably.

This apparatus continued in activity until October, 1818, and I had not yet prosecuted the idea of a secondary compensating pendulum, before I went to Italy, Egypt, Syria, &c. When I returned, after an absence of nearly two years and a half, I was astonished to find, by measuring the tension with the same electrometer which I had used before my departure, and when they were subjected to the same degrees of temperature, that they had not lost more than a twentieth part of their force.

It remains to state what seem to have been the latest and best methods of making the columns, and to preserve them when made.

Mr. Singer almost doubled the original effect of M. De Luc's columns, by interposing *two* disks of paper between the associated metals, upon one of which he placed silver leaf, by attaching it to squares of paper first, with water *only*, allowing these to dry, and then stamping out the disks. His zinc was rolled very thin: it was softened (afterwards, I believe), and polished: and being also punched into disks, which were flattened, they, with the disks of silvered paper and plain paper, were put into glass tubes, in a very dry state, and the tubes hermetically sealed with brass caps. Two out of six of Mr. Singer's columns, which I had from him in 1814, are each still capable of producing a divergence of an inch in Bennett's gold leaf electrometer, and both together of keeping Hardy's inverted pendulum in motion; the other four have about half the same tension, but less than half the same frequency. The diameter of his groups was about seven-tenths of an inch.

Mr. Forster, in Tilloch's Magazine, vol. xlvii, p. 265, mentions an *electrical paper*, prepared by fixing pounded zinc upon the plain side of embossed Dutch gilt paper; but no comparative experiments seem to have been made with columns so constructed.

Professor Zamboni's last recommendation for the manufacture of electric paper is, that the *carta argentata* (which is paper covered with zinc) should be very thin, not glued, impregnated on the unmetallic side with a solution of sulphate of zinc, and well dried before the manganese be put on; that the operation should be performed in very dry weather, and not in the open air. As the best means of preserving his columns, he recommends enclosing them in tubes of rather larger internal diameter than the disks, and filling up the interval with a composition of wax and turpentine. I saw some columns constructed in this manner at Verona, in 1821, which seemed to possess considerable energy; but as it happened to be the vacation, and the professor absent, I could not learn from himself particulars, as to the length of time which had elapsed since they were made, &c. By all accounts, this mode of construction, when carefully performed, and with strict adherence to the directions given, seems to be very advantageous.

Those groups which are best calculated for producing a great degree of frequency, are of course those of the largest area: therefore, wishing to give motion to a *heavy* pendulum, which should vibrate seconds, part of my columns were constructed, in 1815, on Mr. Singer's plan, except that they are $1\frac{5}{8}$ inch *square*. And they are preserved, as I have said, by being *completely* buried in cement. The series consists of 9000 groups, and are able, at the temperature of 60°, to produce a divergence of 50 degrees of the circle, on a pair of straws $1\frac{1}{2}$ inch long, and $\frac{1}{10}$ inch diameter, and to move a pendulum apparatus, which I shall presently describe. The principal advantage obtained by this arrangement is that of exposing as small an insulating surface to the injuries of our humid atmosphere as is possible; for it is seen (Plate 8, Fig. 1), that the glass tubes L and M, covered with sealing wax, which descend through the cover of the box and the cement, and which conduct the wires to the opposite extremities of the series of columns, are the *only* insulating surfaces exposed to the air; whereas the exterior surfaces of *tubes* containing columns expose more surface than the surface of the edges of the disks themselves.

I am not aware, that any improved mode of construction has been devised since Mr. Singer's and Sig. Zamboni's; but having paid little attention to the subject latterly, it is possible that I may have omitted important ones.

Everybody, who has given it consideration, will no doubt admit, that those columns which are put together in the *driest state*, and which contain the least oxidizing and least oxidizable materials, must be the most durable, but *at first* the least powerful.

Several attempts have been made to regulate the motion of the pendulum by connecting it with watch escapements, I believe, but no success seems to have attended them. To state the many fruitless experiments which I have made, with a view to obtain a compensation for the effect of heat, &c. upon the columns, would be to expect almost as much patience from the reader as the experiments themselves required; but having now before me a pendulum, vibrating seconds as *regularly at least* as any common clock (the result of some trials made in conjunction with a most obliging and ingenious friend), I shall describe it as shortly as possible.

A is the box of columns, B is a strong pillar of wood, C is the support of the *compensation* pendulum D, which is made of very light glass tubes, and suspended by a pivot furnished with steel points, which are received by cavities in the ends of two screws. E and F are disks, cut out of very thin latten, weighing only four grains, and of one inch diameter, suspended by *very* fine flexible soft gold wires from the arms of the pendulum. G and H are plates of brass, insulated by glass supports, and connected, by means of wires, each with the opposite extremities of the series of columns contained in the box, for these wires pass through the glass tubes L and M. N and O are small weights, suspended upon the wires, in order to secure their good contact with the plates G and H, and a fine wire, I, places the two extremities of the arms, which in every other part are covered with sealing wax, in conducting communication with each other. The weight of the whole pendulum is two hundred and fifty grains; the distance of the bob from the point of support eleven inches; and the length of each arm four inches and a half.

1. When the disks E and F alternately touched the plates G and H (which attracted them by virtue of their opposite states of electricity), the fine wires which suspend them allowed, by their extreme flexibility, the pendulum to complete its vibrations (or very nearly) in regular time.

2. When we removed the plates G and H, so

that *no* electric attraction was exerted, the number of vibrations in a minute was exactly the same as before.

3. When the electrometers connected with the two extremities of the series of columns displayed an increment of tension equal to about a sixth part of the original divergence (which increase we produced by raising the temperature of the room seven degrees and a half), no difference occurred in the number of vibrations in a minute.

4. When we employed a large insulated Leyden jar, charged to such tension that it was capable of giving a spark of a twentieth of an inch in length from either coating (which was a force at least twenty times greater than the columns were capable of producing), and connected each coating respectively with the plates G and H, the pendulum, instead of being accelerated, seemed rather on the contrary to be *retarded*, in about the usual ratio which the retardation of common pendulums bear to the increase of the arc of vibration.

5. When the jar was charged as highly as it was possible without a discharge taking place, through the spaces of air comprised between the fixed plates and the moveable disks (a distance of about one-third of an inch), it seemed that the number of vibrations in a minute *increased*

in some proportion to the rate of diminution which the charge gradually experienced.

These experiments were repeated many times, and always afforded nearly the same general results; but we fear, that the apparatus was too hastily constructed, and too many distracting causes may have influenced its action, to permit us to insist very confidently upon all the deductions which we drew from them. Still we think it *fair* to infer, that the moderate changes in the energy of the columns (when compared with the enormous difference between its greatest power and that of our charged jar) would not produce any very material irregularity in the vibrations of a pendulum thus constructed *carefully;* particularly were silk thread, as it comes from the worm, used (as it was in some of the trials) instead of the fine wires and the disks united by a fine gold wire, or were disks of gilt wafers suspended by chains of the finest possible gold or platina wire.

The rigidity, or friction of the fine wires, threads, or chains, which suspend the disks, always, perhaps, opposes one obstruction to the free vibration of the pendulum; and the weight of that disk, which happens not to be in contact with one of the plates, G or H, at the moment when the other is, opposes another, I think; but these obstacles, when calculated in comparison with the momentum of the bob, the length of the lever, CD, and *the accelerated velocity at the moment before contact*, amount to mere trifles; and whatever the increased force of attraction, excited by *electric columns*, may at any time amount to, the *effect* of that increase can scarcely do more, perhaps, than give the pendulum a little larger arc of vibration, and of course cause the wires, threads, or chains, to *yield* a little more to the impulse; so that, whilst they continue capable of moving it *at all*, it will *nearly* vibrate in regular time: in short, we venture to conclude, that some such method as this, of compensating the effect of heat, or of any other cause which may influence the energy of the electric column, may be rendered both practicable and efficient.

Had not the above attempt succeeded beyond *my* expectations, and had time permitted, we should probably have tried the effect of employing a secondary *flexible* pendulum (as Fig. 2, plate 8), fixed to and suspended from the same arbor or pivot as the first, or prime mover; or, as in Fig. 3, standing when at rest perpendicularly (a Hardy's inverted flexible pendulum); or, as in Fig. 4, we should have placed the first horizontally, and suspended the second. In all the figures I is an inflexible pendulum of glass; F a flexible pendulum of steel; A the arbor or axis, which supports both pendulums (one behind the